Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

THE CENTRIPETAL AND CENTRIFUGAL DIRECTIONS OF SENSIBILITY IN THE NERVES. At the meeting of the Soc. de Biologie, Dec. 16 (rep. in *Gaz. des Hopitaux*) M. Bert announced that he had for a dozen years been seeking to answer experimentally the question whether shock communicated to the nerves could pass both ways, centripetally or centrifugally. In other words, whether a pinch on the arm, for example, communicates to the nerve at once a centripetal and centrifugal shock. The first was long ago answered, the second still remains to be determined; but to do this it is needful to place, in some way, a brain at the finger-tips. The following experiment for this purpose was performed by M. Bert. Having skinned the end of a rat's tail, he bent it backward on the back of the animal and inserted its tip into a little wound there, so to make the tail, as it were, take root there again. After a sufficient time had elapsed for the circulation to be well established between the exrtemity of the tail and the back in which it was fixed, M. Bert divided the tail into two equal parts, one in its normal position, the other grafted into the centre of the back. Then testing the sensibility of this last portion, he at first obtained no result, but a few days later when its end was pinched the animal turned and attempted to bite, not the end, but the point where it was fixed in the back. From this experiment he inferred that sensibility travels normally over the nerve fibre, either centripetally or centrifugally.

Nevertheless, the objection had occurred to him that the nerves so divided might have become regenerated, as is observed to be the case after surgical operations. He therefore renewed the experiment, taking care to wait eight months before dividing the tail of the animal. Allowing then that the sensibility in the grafted end of the tail was due to regeneration of the nerves from the wound in the back, we ought, in testing it, to find it immediate. But this was not the case, after the section the grafted portion of the tail was insensible up to one centimetre of the back, and it is only after the second day that it becomes sensible over its whole extent. On the second day the sensibility is even exaggerated, and the animal was very sensitive to pinches of the end of the new tail. M. Bert therefore believed his former conclusions to be correct.

M. Onimus observed that the sensibility noted by M. Bert in this exper-

iment, might well be due to some nerve filament near the cicatrix, and was then simply a centripetal sensibility. What led him to this opinion was the exaggeration of this sensibility two days after the section. It is, in fact, the same phenomenon we observe in parts even destitute of nerves, such as cartilages and tendons, in the midst of inflamed tissues.

M. Henocque remarked, relative to the comparison made by M. Onimus, that, up to the present, there is no fact proving the sensibility of tendons. The examples that have been given, relate all to sections around the tendons, and the sensibility observed in such cases is due to nerves in the immediate vicinity of the ligaments, but it is well demonstrated today that no nerves exist in the tendons.

M. Pouchet, returning to the communication of M. Bert, said that it did not seem possible to him that the same nerves can conduct sensibility in two directions, both centripetally and centrifugally. It is not on the nerves, but on individual nerve tubes that experiments must be performed to clear up such a case as this.

In the discussion which followed, M. Bert stated that he intended to make a careful microscopic examination of the nerves in the grafted portion of the tail.

At the meeting of the Society de Biologie, Jan. 6, (rep. in *Le Progres Médical*) M. Bert again brought up the subject. He had completed this physiological study by an experiment that was absolutely conclusive. He divided, some centimetres from its base, the tail of a rat bent backward and grafted into its back. He then determined directly after the section, and the two days that followed, that the trunk of the tail adherent to the back was perfectly sensible, but this sensibility then disappeared. Further, histological examinations by M. Ranvier showed that the nerves of the trunk were completely degenerated when the sensibility disappeared. This experiment showed, therefore, that the nervous influence was propagated toward the peripheral end of the nerves of the tail, which were found to be in connection, at the point of union with the back, with new sensory centres, but had lost their trophic centre.

At the meeting Dec. 24, M. Laborde recalled the subject, disputing the conclusions of M. Bert. He said "M. Bert sought to find if there was a double current in a sensory nerve, if a shock communicated to one of these nerves was transmitted at once in both directions, to the centre and to the periphery." He at first was astonished at this question, since we all know that the sole function of the sensory nerves is to transmit sensations to the centre of perception.

In thus grafting the tail of a rat to its back, what had M. Bert accomplished? He had to admit that, basing himself on his own researches, the sensory nerve, or nerve tube, united itself with another similar one passing to the cord. But was there even such a nerve so disposed, going from the cord to the cord. Such a nerve does not exist said M. Laborde. M. Bert's experiment therefore proves nothing.

Admitting this, M. Laborde seeks to find one more satisfactory. He suggests the section in a frog of one sciatic and uniting its superior portion to a sensory nerve of the other leg. Thus we would have a true periph-

eral nerve returning to the cord. But this would be a very difficult experiment to carry out successfully.

M. Laborde thought it difficult to understand how a shock communicated to a sensory nerve is arrested at once at the point where received, without centrifugal transmission. Clinically he thought, that we have some proofs to the contrary. When, for example, we strike our elbow, we feel not only at the point struck but all along the course of the ulnar nerve, a painful sensation, even at the finger-tips.

M. Gubler did not agree with M. Laborde that the two currents do not exist in the sensory nerve, he thought on the contrary that it was even needful to admit this. As regarded M. Laborde's illustration of the blow on the elbow, from which he deduced the centrifugal as well as the centripetal conduction of the nerve to indicate rather a kind of reflex action, analogous to that observed in amputated limbs.

M. Gubler then mentioned a fact not well known, but which he had constantly had occasion to observe. If we irritate any point whatever of the body, we feel more or less pain at the point irritated, but if the sensation is very intense there is instantaneously produced another pain at a distant point but always at the same. If, for example, the pain is felt in the thigh the other will be felt at the waist; if the primary pain is in the side, the secondary or sympathetic one occurs in the shoulder or arm. In a number of observations by M. Gubler on the same individual, the sympathetic pain was uniformly at the same point. Now it is impossible, he said, that this secondary pain can be the result of a shock communicated to the nerve conducting the primary pain, and he proposed the following hypothetical explanation. There is, he thought it necessary to admit, something analogous to a reflex action here, that is, a sensation conducted to a perceptive centre, an impression received at that point, and this centre reflecting this impression to another point than the one from which it was received. But this secondary current can, like the primary one, pass only over a sensory nerve. These nerves therefore conduct sensibility both centripetally and centrifugally.

M. Laborde considered the illusions of people who had suffered amputations as being nothing in the line of reflex action, and proving nothing in favor of centrifugal sensory currents.

M. Gubler, in reply to a question of M. Pouchet, whether these secondary painful points were not individual peculiarities, stated, that as far as his observations had extended, he had always found them in the same point, and that, the location of the primary pain being known, it was easy to name the place of the occurrence of the secondary one. In other terms, the locality of the two corresponding points are the same in all subjects.

[The above discussion, as reported, indicates what appear to us to be rather strange physiological misconceptions on the part of some of the speakers. If M. Bert's experiments indicate anything, they show that nerve fibres conduct impressions indifferently in either direction, and that their functions depend upon the character of their central or peripheral terminal apparatus. Thus sensory fibres might conduct impressions in either direction, but for the fact that sensation is always a central phe-

nomenon, and therefore, the conductions of its nerves must be centripetal. The anatomical data in our possession are sufficient to render this probable *a priori*; but there are also good physiological evidences in its favor. Thus, when MM. Vulpian and Phillippeaux united the central portion of the divided lingual with the peripheral end of the divided hypoglossal, they united two receiving apparatuses, so to speak, but, instead of their both being of the same kind—sensory—as in M. Bert's experiment, one was motor and the other sensory. The result, as might have been expected, was a suppression of all function, except when aroused by external irritation along the common line of connection between the two. Then movements of the tongue were observed and, had the observers been able to note accurately and completely the subjective sensations of the animal, they would doubtless have found that the sensory centre of the lingual was also excited.

It is not to be inferred that no centrifugal influence is conveyed over sensory nerves—but it cannot be sensory. There are many facts, and among them the one developed by M. Bert in the above experiment, that seem to show that trophic influences pass out over the same routes that convey sensory impressions to the centres. But unless we are willing to admit that consciousness exists throughout the nervous system, in its peripheral as well as its central portions; it seems to us as unreasonable to speak of centrifugal sensory conduction as it would be to talk of centripetal motor impulses acting upon central ganglion cells.]

Cerebral Localizations.—A note by M. Maurice Raynaud was offered at the Acad. de Médecine of Paris at its session, Dec. 5, 1876, (rep. in *Gaz. des Hopitaux*), called out by the memoir of Dr. Proust on cerebral localization, in which he gave the history of a consumptive patient, in whom, three days previous to his death, there appeared suddenly a paral ysis limited to the left superior extremity, and affecting exclusively the hand of the fore arm. At the autopsy, the only lesion discovered was a very minute patch of red softening, developed in the right hemisphere, around a meningeal tubercle. This patch, which was only as large as a twenty centime piece, was situated on the ascending parietal convolution, and in the gray substance forming the base of the fissure of Rolando, five centimetres from the superior internal border of the hemisphere. This point is precisely the one that has been experimentally demonstrated to be in the ape, the centre in relation with the movements of the inferior member.

This observation is the only one in M. Raynaud's knowledge, the only one in which the brachial paralysis existed, to the exclusion of every other cerebral phenomena, permitting the establishment of a certain convolution between the functional trouble and the anatomical lesion.

The following are views of M. Onimus, as recently expressed by him at the meeting of the Soc. de Biologie, Feb. 10., (rep in *Gaz. des Hopitaux*):

M. Onimus said that there were many objections to be made to the hypothesis of these localizations. In spite of the tendency that had pre-

vailed since the agitation of this question, to admit the results of MM. Hitzig and Ferrier, after having studied the works of others on the subject for the past three years, and after having repeated many of the experiments, he was convinced that this doctrine was supported by no incontestible physiological facts. It is probable that there are in the cerebral lobes, regions possessing special psychical functions, this is the old theory of Gall, but at the present day, the tendency is to admit motor centres in the cortex. The phrase "cerebral localizations" is also too vague, for we can admit the existence of psychic localizations, such as that of the faculty of speech, without becoming partisans of cerebral localizations in the actual sense of the word, indicating psycho-motor localizations. M. Onimus offers the following objections to the physiological facts that gave rise to this hypothesis:

Every nervous element is put into action either by chemical, physical, or electrical excitations. A motor nerve causes a contraction, and a sensory nerve a sensation, whether the excitation is produced by mechanical pressure, cauterization, or by an electric current. But excitation of the cerebral lobes causes absolutely no motor phenomenon when we irritate, chemically or mechanically, their cortical portions, we may burn them with a red-hot iron without producing any movement. Electricity alone, among all the excitants, produces motor phenomena. Why is this? Simply because its effect does not remain isolated in the point of application like that of other excitations; it penetrates more deeply.

Electric currents are not transmitted by the nerves, but by the organic liquids, which are always better conductors. The diffusion of the currents, when applied to the cerebrum, they diffuse themselves in all directions, particularly downwards into the brain, since they follow the vessels. For this reason, it is near the fissure of Rolando, where the vessels are very numerous and communicate with those of the corpus striatum, that we meet with the pretended psycho-motor centres.

Moreover, after removing the hemispheres from an animal altogether, and replacing them by a mass of bloody matter, M. Onimus had found that the application of electricity in this mass produced the same effects as electrization of the cerebral lobes themselves. If the electrodes were applied superficially to the anterior part of the mass, movements of the eyelids were obtained; more deeply and posteriorly, those of the members were produced. There can be no motor centres here, and the experiment demonstrates very well that currents applied to the cortex penetrate more deeply.

Electrical currents, acting in a general way upon all the nerves, do not affect all alike; some are excited and put into action, while others seem unaffected, or only manifest their function with the use of a very strong current; there is an elective action. Thus, of all the nerves, those of the eyes are most impressionable, next, those of the face, and lastly, those of the members, especially those whose functions are most exercised. Thus, when we electrize the cerebral lobes, the first movements to be excited are those of the eyes, and their motions are even produced when we carry the rheophores over the supposed centres for the members. These phenomena seem to indicate that the results obtained depend upon a general excita-

tion, and not upon any special and local action. We may form an idea of the diffusion and the elective action of the currents, when we remember that in man a very feeble current directed in the neck is sufficient to cause, through the epidermis, the tissues, and the bones, an excitation of the optic nerve. In this case, there is neither cutaneous sensation nor contraction, and yet the current penetrates a considerable mass to excite at a distance the optic nerve.

In the lower animals, destruction of the cerebral lobes causes no disorder of motricity, but even in the higher animals the troubles so caused are very different from those obtained by injuring other portions of the encephalon. A simple puncture, or the presence of a very minute foreign body, suffices to produce considerable modification in the motor symptoms, while in dogs, rather extensive lesions cause only a transient enfeeblement.

Further, we have, from the *ensemble* of the experiments performed upon the cortex, the following curious and contradictory result, that the destruction of the cortical regions, *the electric excitation of which causes contraction of the flexors, does not induce paralysis of these muscles, but rather of the extensors.* Thus, the movement we obtain in the anterior members by electrizing certain portions of the cerebral lobes, are those of flexion and adduction, and if we remove these parts with a curette, the animal exhibits no symptoms, or rather he shows for from two to four days a paralysis of the extensors. This transient paresis is here only the result of the shock to the nervous centres, since it is almost a general law that every momentary enfeeblement, not localized, shows itself by paresis of the extensor muscles of the fore-arm.

Further, every excitative action on the whole of the nervous centres, or of a plexus, causes movements of flexion and adduction; this is likewise the case, in clinical observations.

En resumé it is incorrect to say that the excitation of the cerebral lobes *functionally*, produces motor phenomena, for none of the excitants, except electricity, produce these results. The electrical currents are an exception only because they penetrate more deeply, by reason of the conductibility of the liquids, and they produce the same effects when the cerebral lobes are replaced by an amorphous mass, also a conductor of electricity. Moreover, the transient paralyses, following destruction of the cortex do not correspond at all to the groups of muscles caused to contract by the excitations of these same regions. The only fact that agrees with the experiments on which the theory of psycho-motor centres is based, is that excitations applied to the vicinity of the fissure of Roland, act more energetically on the inferior nuclei than when applied to other regions of the brain.

M. Duret, following M. Onimus, said that there were three methods of experimentation available for demonstrating the existence of cerebral centres: 1. Electrization; 2. Ablations of parts of which we wish to learn the physiological functions; 3. Clinical and surgical observations. The electrization experiments are exceedingly difficult to interpret, on account of the diffusion of electricity; there are, in fact, regions that do not respond to electricity, and he asked a physiological explanation of this fact.

He did not think, with M. Onimus, that the conduction of the vessels sufficed to explain certain phenomena of electrization that were observed. The experiments by ablation, on the other hand, gave us to-day, positive facts and results perfectly clear and well defined.

M. Onimus said he had never any permanent paralysis by the mere ablation of a part of the brain in animals.

M. Charcot observed that certain pathological facts, too well studied, and already very numerous, also were in favor of localizations, and that we cannot counterbalance them by experiments upon animals.

M. Laborde asked to be allowed to add a fourth to the three methods mentioned by M. Duret, for the study of cerebral localizations, one that he had attempted to introduce into physiological experimentation; he referred to the artificial production, in the brain of the dog, of hemorrhages exactly similar to those observed in man. He recalled the fact that he had communicated to the society the results of these experiments, and that he had obtained by this procedure, symptoms exactly like those observed in man in cases of cerebral hemorrhages. These facts demonstrate that there are in the brain of the dog, limited regions, the excitation of which produces perfectly localized phenomena.

M. Lepine remarked that M. Laborde's method was a great advance in physiological experimentation, and that it was much to be preferred to that of Goltz, who had destroyed portions of the brain by a jet of water, at high pressure introduced through a small orifice in the skull.

THE VASO-MOTOR NERVES.—P. Bricon. *Thesé de Strasbourg*, 1876, (abstr. by M. Duval in *Revue des Sci. Méd.*) Examining the mode of action of the so-called vaso-dilator nerves, the author is led to adopt the theory of the autonomous contraction of the vessels (the theory of Legros and Onimus), that is, the theory of peristaltic contraction.

His experiments may be stated as follows: 1. Whenever, after section of a nerve containing vaso-motor fibres, we apply feeble irritation to its peripheral portion (multiple sections, chemical agents, or weak induced currents) we always obtain in a healthy adult animal an elevation of temperature and a dilatation of the vessels of the corresponding member. 2. If the irritation is produced by a strong induced current, we obtain a lowered temperature and a vascular constriction.

Considering the section of the nerve as one of the class of feeble irritations, the author is led to admit that the immediate effect of the operation is an active and not a passive one, that is to say that the peripheral vascular dilatation is not due to paralysis but rather to excitation of the vaso-motors, which, producing peristaltic and autonomous contractions of the vessels, enlivens the circulation. On the other hand every violent excitation produces a spasmodic contraction of the vessels, and consequently a diminution of the sanguine afflux. There is, therefore, but one species of vaso-motor nerves, and the vaso-dilator and vaso-constrictor effects will be due solely to the degree of excitation of the same nerves, inducing sometimes peristaltic action, sometimes tetanic contraction of the

arterioles. There follow some experiments made upon tubes of India rubber, compressing them with a wooden roller, to imitate the compressions produced by the peristaltic action, and to show that these compressions favor the circulation and augment the flow of the liquid.

THE INFLUENCE OF INDIRECT CURRENTS UPON THE VESSELS. — A recent communication by M. Onimus in the Soc. de Biologie (Dec. 30, 1876), is thus reported in the *Gaz. des Hopitaux*. It is known, said he, that electrization of the vaso-motor nerves causes contraction of the smooth fibres contained in the vascular walls, contracts the caliber of the arterioles, and diminishes the circulation. The conclusion has been drawn from this that excitation of the vaso-motors always provokes permanent contraction of the vessels and reduces the circulation. Account was not taken, however, of the excitation, with ordinary induced currents; we have, then, in effect, to deal with a series of rapid irritations that cause a tetanus of the muscular elements of the vessels. If with the same intensity of current the number of the interruptions is varied, the phenomena of vascular contraction and reduction of the temperature diminish alike and in the same proportion. When there are only one, two, or three excitations per second, the temperature instead of being lowered, is slightly elevated, and the circulation appears more active.

Under the microscope, we observe in the natatory membrane of the frog, the capillaries dilating and the circulation becoming more active when we electrize the vaso-motors with indirect currents, interrupted once or twice every second. The same occurs in warm blooded animals. We may conclude as follows:

1. Rapid excitations affecting the vaso-motor nerves cause tetanic contractions of the vessels, and diminish the circulation.

2. These same excitations, when they approach in their order of succession, the normal conditions of the rhythmic movements of the vessels enliven these movements and render the circulation more active.

These facts also confirm the theory of the autonomous contraction of the vessels, since they show that, if indeed the tetanic contraction of the vessels reduces the circulation, successive and *rhythmic* contractions, on the other hand, increase the sanguine flow.

It is important in these experiments, to employ currents of moderate intensity, since with very feeble currents we often obtain, even with rapid interruptions, a slight vascular dilatation. These last facts accord with those observed by M. Goltz and M. Bricon, one of his pupils.

The differences in the number of the excitations in a given time, and the influence of the intensity of the induced current upon the peristaltic movements of the intestines, have a very striking analogy with those observed in the vascular phenomena.

THE DIFFERENCES OF THE ACTION OF THE TWO PNEUMOGASTRICS.—At the meeting of the Soc. de Biologie, Dec. 9, (rep. in *Gaz. des Hopitaux*).

M. Leon Tripier (of Lyons), presented in his own name and that of M. Arloing, a communication on the different actions of the two pneumogastric nerves.

He referred to the fact that in June, 1872, M. Brown-Séquard had kindly presented for them a former communication on the same subject. They had claimed in that paper that the right pneumogastric acted more energetically upon the heart than the left, and *vice versa* as regarded the lung. (See this JOURNAL, No. 1, Vol. I., Jan., 1874). At the same time, M. Masouin (of Liege) arrived at identically the same result as regards the heart. Since then the experiments have been repeated by M. Tarchanoff. A little later they published in the "*Archives de Physiologie normale et Pathologique*" the whole of their researches on this subject, and in the part that more particularly related to the heart, they always admitted the fact of the inequality of the action of the two pneumogastrics, but as they had found in a few cases a predominance of the left vagus, they were less positive. They also gave more details on the modifications produced in the respiration after division of the left pneumogastric. Finally they stated that all their attempts to discover a difference of action of the vagi on the œsophagus and stomach were fruitless, or rather indecisive.

Nevertheless, at that time they had observed two cases of death after section of the right pneumogastric in the ass; but this had caused no surprise, it had been long admitted that death sometimes followed section of the two pneumogastrics. Observing the relative frequency of this event, they sought to ascertain the ratio of this frequency. In twelve sections in the ass, death occurred seven times, in four of which it was the right, and three the left pneumogastric that was cut. In nine sections of the nerve in the rabbit, death occurred three times, the right nerve having been operated upon in all three. In more than forty operations on the horse, it occurred only once, and then it was the right nerve.

From these we conclude that division of one vagus may cause death, and that division of the right nerve is more likely to produce this result than that of the left.

In all these cases the stomach was found filled with food, and particles of the same were met with even in the air-passages. This is very well explained by the paralysis of the lower part of the œsophagus in section of the pneumogastric in the middle cervical region. It is none the less true, that we know as yet, relatively nothing of the causes and nature of the phenomenon. Therefore, MM. Arloing and Tripier hesitate to pronounce upon it. But whatever they may be, these facts have a double interest, both in a physiological and a practical point of view.

In the discussion that followed, M. Tripier stated, in reply to a question of M. Trasbot, that he had not specially observed for any pulmonary lesions following the divisions of the pneumogastric that might cause death, but had had in view the difference of action of the two vagi nerves.

M. Claude Bernard rejected Traubes explanation of these phenomena, that in these cases the animal died from the results of the admission of foreign bodies in the air passages, setting up inflammations and abscesses.

But if we divide the two vagi in a rabbit, and then introduce a tube into the trachea, in such a way as to allow it to respire freely and prevent any foreign substance from entering, the animal still dies with patches of pulmonary disorder, and the quicker the younger it is. It is a question of the resistance of the lung; moreover, all the questions attaching to this subject are exceedingly complex.

To an objection of M. Trasbot, M. Claude Bernard said that he did not deny that animals might sometimes die in the way indicated, but that that was by no means always the case, and the question was a very complicated one, and required much more study.

M. Lepine asked M. Tripier if he would not admit, to explain the difference between the action of the two pneumogastrics, either a greater number of fibres in the right, or, what is perhaps more probable, a difference of action of the two cerebral hemispheres from which the nerves arose, and a sort of preponderant influence of the left hemisphere over the right, explaining the preponderance of action of the right nerve, there being a decussation of the pneumogastrics as of the other nerves. There would be, therefore, something analogous to what we observe in aphasia.

M. Tripier replied that he had begun, with his collaborator the study of the mechanism, but that he was not yet able to report upon it, and that in this point he agreed with M. Claude Bernard.

THE NERVES OF THE OVARY.—The following is a preliminary communication by Dr. Julius Elischer in the *Centralblatt f. d. med. Wissensch.* No. 50, 1876, on the finer details of the ovarian nervous supply.

While the anatomical course of the ovarian nerves may be considered, as decided by the researches of Frankenhaeuser, a knowledge of the finer histological details of these elements has hitherto been lacking.

The majority of investigators have not agreed as to the presence in, and the entry with the vessels into the hilus of the ovary, of the nerves, and hence it seemed a useful task to seek out the nerves in the stroma and to follow them to their terminations, which I have done so far in the rabbit, the sheep, and the cow.

The best method has been, hardening the specimens in a two per cent. solution of chromate of ammonia for a short time, changing the fluid daily, then coloring with gold chloride, according to the Gerlach-Boll formula, and finally the restoration of the dissected preparation to the hardening fluid again.

In all the animals examined there were found fine twigs of medullated nerve fibres entering the stroma with the tortuous vessels, and also in the hilus as well as in the ligamentum ovarii proprium. These branched off from the hilus after two fashions. While one division passed as medullated fibres to the follicular layer of the periphery, and there began to branch dichotomously into a finer and finer network of non-medullated fibres, that are sometimes extended directly around the follicle, but that sometimes form very tortuous curled branches,--another division remains

as a coarse-meshed network, visibly encircling the vessels. The riper the follicle, *i. e.*, the more developed its granular membrane, the more prominently the separate parts of the follicle are seen and simultaneously there is formed in the theca folliculi a coarse meshed numerously anastomosing network of tolerably large fibres, out of which an elongated meshed fine fibred network arises and lies upon the peripheral layer of the membrana granulosa. This last, through its branchings, forms a dense network which surrounds the membrana granulosa and which is easily recognized as a nerve plexus by numerous knots and varicosities. It is somewhat more difficult to find places where twigs from this plexus enter into the cells of the granular membrane.

I have, nevertheless, seen many cells, of the granular membrane (distinguished by well-marked processes, etc.) into which the nerve twig entered and disappeared in the nucleus.

The most suitable object for examining the nerve terminations is the ovary of the sheep, and with care as to methods the plexuses I have above sketched, may be found without too much trouble, especially if the remaining deposit of gold chloride is cleaned off from the section with a brush in oil of cloves.

THE CORRELATION BETWEEN THE DISTRIBUTION OF THE CEREBRAL ARTERIES AND THE PHYSIOLOGICAL REGIONS OF THE BRAIN.—At the session of the Soc. de Biologie, Jan. 6, (rep. in *Le Progres Médical*) M. Duret exhibited designs and injections intended to show that there exists a great correlation between the distribution of the arteries and the physiological regions of the brain. As in man, so in the dog, cat and rabbit, the territory of Sylvian artery corresponds very nearly with the situation of the voluntary motor centres described by Ferrier. Hence we may divide the brain into three great regions, having the same limits as the vascular territories, the motor, the sensory, and the intellectual regions, corresponding to the distribution of the Sylvian, anterior cerebral, and posterior cerebral arteries, respectively. The lobes and the convolutions are only accidents of the region, playing a secondary *role*, M. Duret took up the study of the development of the brain in this point of view; he attributed a preponderant influence to the mechanical action exerted by the cranium on the encephalic vesicle at the moment of the formation of the folds of the hemispheres. It is necessary to say that, the smaller the facial angle, the nearer to the anterior parts of the skull is the fissure of Rolando in man, and the crucial sinus in the lower animals. The skull has already its definite form and is resistant when the encephalic vesicles are still smooth and membraneous, compressed between the anterior and posterior halves of the skull, they fold themselves in the direction of the slightest force, that opposite the base of the two cerebral nuclei, already formed at this time; the smaller the bony skull, the more direct is the force it exerts, and the more anterior are situated the fissures of Rolando and the crucial sinus.

There is a third point insisted upon by M. Duret. There exists for the third convolution in man, a special artery, met with in all animals whose

brains have been studied, and it occupies an analogous position, even in those whose brains are smooth; it corresponds to the centres described by Ferrier, for the movements of lips and tongue. Following this idea, M. Duret extirpated this region in three dogs, to see whether the same phenomena as those observed in man after injuries of the part, would follow the lesion. Although the result was not quite definite, it still appeared that two of the animals had lost the power of barking.

THE RAPIDITY OF SENSORY PERCEPTIONS.—M. Chas. Richet reported to the Soc. de Biologie, Dec. 2 (rep. in *Gaz des Hopitaux*) the results of his studies in sensibility by experimentation on healthy individuals. From these he laid down the following propositions:

1. The sensibility, put in play by a feeble excitation, decreases slowly, but rapidly returns to a normal condition after even a very short period of rest.

2. Multiplied excitations produce a sensory effect beyond that which a single excitation can incite, this fundamental fact can be explained only by addition (summation of Gruenhagen and Pflueger), by which the successive excitations accumulate in the brain.

3. The direct consequence of this proposition is, that slignt excitations are very slowly perceived, while stronger ones are nearly instantaneous: this is true only for series of electric excitations, but is not correct for single excitations, which, whether strong or weak, are felt at the same time.

4. A relation exists between the frequency, the intensity and number of the excitations, thus the number of the excitations necessary to provoke a perception, is in an inverse ratio to their intensity and frequency.

5. The persistence of an impression in the nervous centres is in direct ratio to the intensity of the excitation that produced it.

From all these facts it follows that we must distinguish the excitation of the nerve and the excitation of the nerve centres. Excitation of the nerve is a simple transmission; excitation of the centres is a sort of shock, of vibration that endures a long while after its primary excitation. In this point of view the nerve centres resemble the muscles, and there is a direct relation, not between the functions, but between the form of the functions of these two tissues.

THE ELECTRIC IRRITABILITY OF THE FROG'S BRAIN.—O. Langendorff. *Centralblatt f. d. Med. Wissensch.* No. 53. 1876.

A series of experiments performed by the author upon the electric irritabilitty of the cerebrum of frogs have led to the following results:

1. By irritation with weak, constant or interrupted currents of certain portions of the hemispheres, we may induce movements of the muscles of the body.

2. By simultaneous irritation of both hemispheres we produce movements of all four members and of some of the muscles of the back. Unilateral excitation produces movements of the body and of the extremi ties of the opposite side.

3. The "irritable zone" lies in the parietal section of the hemispheres. Irritation of other parts produces no effect when weak currents are employed.

4. After complete separation of the cerebrum from the parts of the central nervous system lying behind it, the results of irritation of the hemispheres disappear.

5. Ether narcosis destroys the electric irritability of the cerebrum. On the other hand it is not affected by complete removal of the blood from the body of the frog.

6. There is one point in the uninjured skull of the frog, electrical irritation of which produces the same effect as direct application of the current to the same side. This point lies between the tympanic membrane and the eye, and can be easily located by a suture-like dark line connecting the eye and ear.

The author is still engaged upon his experiments and hopes soon to give further results.

THE PHYSIOLOGY OF THE PERSPIRATION.—At the session of the Berlin Physiological Society, Jan. 26, Herr Adamkiewicz offered a communication upon the physiology of the sweat secretion which is thus reported in the *Deutsche Med. Wochenschrift.*

Physiology is acquainted with two causes for secretion within the glandular substance;—physical force causing the processes of filtration and physiological causes acting through the direct mediation of the nerves. Hitherto, we have had only one experimentally confirmed example of this second kind of agency in the production of secretion, that of the submaxillary gland and its functional relations with the excitation of the chorda tympani and the sympathetic. Herr A. had been able to demonstrate by experiments upon men that the sweat secretion is also a nervous act which may be induced by artificial irritation of centrifugal nerves (Luchsinger has recently arrived at the same conclusion from experiments upon animals). These nerves follow the motor routes of the regions concerned and react, under favorable conditions to irritation, for which the influences of temperature are of importance, within less than one minute. They have a common centre for the lower extremities, in the spinal cord, are not directly dependent in their action upon the circulatory processes, and indicate that the process of the secretion of sweat is a simple reflex act, obeying the well known laws of these actions, and properly to be classed among the bilateral reflexes.

NUCLEI OF THE GANGLION CELLS—G. Schwalbe, *Jenaische Zeitschr. f. Naturw X.*, 1876, 25, (abstr. by Loewe in *Centralbl. f. d. med. Wissensch.*).

If a fresh, still perfectly transparent retina of a sheep is carefully spread on the object platform, with its inner surface in the vitreous humor, upwards, it is easy to perceive the ganglion cells, well defined, in a fresh state, in the peripheral parts adjoining the ora serrata, since here the mass of nerve fibres are reduced to scattered bundles. We recognize-

imbedded in a smooth, shining homogeneous mass, certain round, clear spots, which seem as if filled with fluid. Closer examination reveals within these clear spaces a circular nucleus, having every appearance of a ganglion cell nucleus. The whole surrounding space is clear, with the exception of a little circle of finely granular substance around the nucleus. The addition of iodized serum renders the whole transparent space turbid, and apparently finely granular. As an argument against the supposition that the granular layer is nervous, Schwalbe adduces the entirely different optical condition of fresh ganglion cells, and fresh granular substance. The latter appears as if penetrated with numberless little vacuoles. The shining homogeneous substance between the ganglion cells, allows no recognition of any trace of form elements, and is plainly comparable to the cementing material of the epithelium. The nuclei of the ganglion cells of the retina possess a membrane, the inner surface of which is furnished with little prominences. Often they contain a pointed nucleolus, furnished with feathery processes. This is formed of the same nucleolar substance as the nucleus membrane and its excrescences. In adult animals the differences in the size of the ganglion cells are proportionately slight. In young animals (calves) they are, on the other hand, extraordinarily large, and the nuclei in proportion. The smallest nuclei are the youngest. They have no trace of nucleolus, and consist of a uniformly granular mass. A differentiation into nuclear membrane and contents is impossible. The substance which later forms the membrane and nucleolus, is, at the beginning, uniformly distributed through, and fills the whole nucleus, and is filled with little cavities containing another substance. With the growth of the nucleus, the vacuolar substance increases, without, at the same time, any perceptible addition to its other constituents. The result is that the latter is divided into different portions, of which one always occupies the superficies of the nucleus to form the so-called nuclear membrane, with numerous pointed projections, the mural nucleoli which project toward the interior of the nucleus, while other portions gather into one or more nucleoli. In the measure, as the clear substance within the nucleus increases, so the inner prominences of the nuclear membrane constantly flatten out in consequence of the increased extension of the latter. The whole process may be looked upon as a vacuolizing similar to that observed in vegetable cells during the separation of the protoplasm from the cell fluids. In ganglion cells in other localities (anterior horns of the spinal cord in rabbits and pigs, the gasserian ganglion of rabbits, the spinal and sympathetic ganglia of the frog), the nuclear membrane is lacking, and also the mural nucleoli, (*i. e.*, the thickenings of the nuclear membrane on its inner surface). The clear nuclear fluid with a vacuole containing nucleolus, immediately borders the cell substance. This observation of Schwalbe is not in agreement with the statement of Auerbach, that the nucleoli enter the nucleus from the cell protoplasm. According to Schwalbe, this, like the nuclear membrane, is formed from the original nuclear substance, since the latter, through the accumulation and increase of the transparent nuclear fluid, is ruptured in several places. There is, moreover, no increase of the nucleolar substance; this remains the same,

and even relatively decreases in comparison to the growth of the nucleus Hence, it follows that we see in the ganglion cells, in opposition to Auerbach's statements as to other cells, a multi-nucleolar condition precede a uni-nucleolar one, and that this latter may pass into an a-nucleolar condition, in which the general nucleolar substance is changed into nuclear membrane. Schwalbe comes to the conclusion that Auerbach's statements as to the formation and increase of nucleoli, are not of general application. In the bodies of the spinal ganglion cells of the frog there are two substances, one of which forms a very fine net-work that extends from the membraneless nucleus to the external cell wall, while the other more transparent substance fills its interstices. The substance of the nuclear granules appears optically different from both; on the other hand, the nuclear fluid seems identical with the substance, filling the interstices of the meshes of the net-work. We have, therefore, three substances to distinguish in the ganglion cell—the nucleolar substance, the nuclear fluid (cell fluid), and the reticular substance. The opinion of Max Schultze as to the fibrillary constitution of the nerve cell, depends, according to Schwalbe, on an imperfect appreciation of the reticular substance. In conclusion, he calls attention to the variations in the structure of the ganglion cells from different localities.

NERVOUS SYSTEM OF THE CETACEA.—At the last meeting of the British Association for the advancement of science (rep. in *Nature*, September 28, 1876), Dr. D. J. Cunningham read a paper on the Spinal Nervous System of the Cetacea. He found that while great similarity prevailed between their cervical and dorsal nerves, and those of other mammalia, the nerves of the lumbar and caudal regions differed widely. The superior and inferior divisions of those nerves in cetacea, were of nearly equal size. Two great longitudinal cords, or trunks, are formed by their union on each side of the vertebral column, and these become situated on either side of the spines of the vertebræ, and on either side of the bodies below the transverse processes. These great cords supply the four great muscular masses which act upon the tail.

The following are the titles of some recent papers on the Anatomy and Physiology of the Nervous System:

VINTSCHGAU and HONIGSCHMIED, Experiments on the Reaction-Time of a Gustatory Sensation, *Pfluegers Archiv.* XIV., XI. and XII, 529; KUEHNE, The Coloration of the Retina and Photography in the Eye. *Rev. Scientifique*, March 3; LAUTENBACH, The Conducting Power as Distinct from the Receiving Power of the Nerve, *Phil. Med. Times*, March 17; CHARCOT and PITRES, Contribution to the Study of Localizations in the Cortex of the Cerebral Hemispheres, *Rev. Mensuelle de Médicine et de Chirurgie*, January, February and March, (Cont.Art.); GRANVILLE, Ideation in Utero—a suggestion; *Lancet*, March; MARCACCI, Determination of the Excitable Zone in the Sheep's Brain. *Archivio Italiano*, January.